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Is Truth in Authority or Authority in Truth? Limitations to the Publication of Scientific Research

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ABSTRACT

Fisher J, Steele J. Is Truth in Authority or Authority in Truth? Limitations to the Publication of Scientific Research. **JEPonline** 2012;15(1):57-64. This paper examines the limitations and potential bias that exist within efforts to publish articles in exercise physiology. By discussing perceptions that: (a) greater truth appears to exist based on the publishing authority or journal title, (b) that some organizations appear reluctant to change or progress their philosophies and thus recommendations, (c) that there is a large potential for bias in the peer review process, (d) that many research articles may end up in "the file drawer" unpublished because of their apparently insignificant findings, as well as (e) the importance of the impact factor, and (f) open access journals, we hope to enlighten young authors and remind experienced peers that science should be nothing more than an attachment to the truth. We believe the unbiased processes considered are invaluable in the scientific publication process, but that both perceptions and evidence presented herein support that the limitations exist and need consideration.

Key Words: Bias, Peer Review, Impact Factor

TABLE OF CONTENTS

INTRODUCTION

The difficulties endured by advancing scientists date back centuries, and while the genius of Galileo Galilei and Albert Einstein might be exceptional, the opposition to ethical scientific process appears far more frequent. Martin (12) discusses exactly this point: that newcomers or progressive arguments are often ignored, ridiculed or rejected, or worse – attacked. Conceivably, in the past, scientists must 'earn their stripes' before their work was to be considered; evidence perhaps of a relationship between the number of stripes required and the more unconventional theories. The reality that this might still exist is at the least disappointing to some, and disturbing to others. Might we have evolved to perceive validity by name; that we accept an understanding of evidence not by research itself but rather by the scientist?

With this in mind the purpose of the present article is to discuss some of the limitations of the publication of scientific research. The following subheadings should be discussed openly and without fear of reprisal. Young scientists and academic researchers will potentially experience, or at the least witness, the limitations contained herein. The awareness derived from this article might serve to instill a resistance against potential bias and, therefore, reinforce the scrupulousness of their research, their publications, and their peers.

THE TRUTH, THE WHOLE TRUTH, OR NOTHING LIKE THE TRUTH

It might be argued that conflicting opinions exist as a result of interpretation, contrasting scientific evidence, and/or further research; all are both healthy and necessary in a search for the truth. A recent article reported that 13% of publications in the *New England Journal of Medicine* were in fact reversals[1] of established concepts (14). This report suggests that new evidence can progress our knowledge and re-write previous perceptions. Of course this evolution in scientific process and theory is somewhat expected, but it is also occasionally dismissed. For example, Carpinelli (4) explains in considerable detail his opinions of the misrepresentation of research as well as the potential bias displayed by the ACSM in their 2009 position stand: *Progression Models in Resistance Training for Healthy Adults* (15). Indeed, to clarify Dr. Carpinelli was initially a reviewer for the 2002 Position Stand (9) but was later removed when he challenged many of the references; a questionable ethical maneuver to say the least. There is a vast body of research that appears to contradict many of the

[1] Reversal is defined as "the phenomenon of a new trial—superior to predecessors because of better design, increased power, or more appropriate controls—contradicting current clinical practice" (14).

ACSM's recommendations (3,6). While this article is not aimed at supporting or targeting any author or organization, it would surely appear logical to rebut these comments, and attempt to explain why differing research exists and, therefore, reinforce the evidence as to why the ACSM proposes its recommendations.

Ultimately, the criticisms of research findings and/or the scientific publication process exist to allow difficult questions to be answered; either strengthening an argument or breaking down weak conclusions. While acknowledging that new evidence might go against years of research, one might perceive that an inability to accept this change is indicative of a shift away from the scientific process and all that it stands for. To quote Alvin Toffler (24), "The illiterate of the twenty-first century will not be those that cannot read or write, but those that cannot learn, unlearn and relearn." In this case, it is likely that 'unlearn' is simply a reference to replacing existing theories or methods, earlier stated as 'reversal' by Prasad et al. (14). Indeed, Darwin (5) once clarified, "it is neither the strongest of the species, who survives, nor the most intelligent, but the one most responsive to change."

Whilst institutions and authoritative bodies might be slow to adopt change, resistance likely should not come from editorial positions. The ACSM's 2009 position stand (15) was preceded by an editorial by the Editor-in-Chief of the publishing journal, *Medicine and Science in Sports and Exercise* (MSSE), who attempted to clarify conditions as to which the journal would accept criticism (26). However, these conditions were difficult to interpret. It was highlighted that letters of criticism to the underlying science used to support the ACSM's position stand would be accepted, but letters directly questioning the ACSM's official position would not be considered. This would seem peculiar as any criticism of the underlying science would without doubt lead to questioning of the positions that the ACSM had derived from that science. While ultimately unclear as to the appropriate circumstances with which critique might be considered, the question of course is that if the ACSM is presenting scientifically sound guidelines based on evidence, then, why would MSSE be reluctant to accept any opposing views or criticisms, since they would afford both MSSE and the ACSM a rebuttal which would surely serve to strengthen their original publication. Of course, we might go so far as to ask why such a criticism could even be possible based on the presumably stringent editing of the ACSM authors, as well as the reviewers for MSSE.

THE PEER REVIEW PROCESS

As researchers and reviewers, we adamantly believe in the peer review process that employs methods to avoid bias (16). Indeed, Rowland (18) describes the peer review process to include four main functions: (a) dissemination of current knowledge; (b) archiving of the canonical knowledge base; (c) quality control of published information; and (d) assignment of priority and credit for their work to authors. Rowland (18) continues with discussing the evolution of the process to generally include two reviewers and the editor (who might potentially act as a third reviewer or has the opportunity to reject the article based on other grounds). However, as researching exercise physiologists, we worry that governing bodies and their associated newsletters and journals hold sacred their philosophies, and as such any research that appears to oppose or question their opinions, or simply not use their favored protocol could be judged less favorably when submitted to one of their publications or reviewed by one of their members.

Indeed, the editor-in-chief of any journal has the power to reject an article without sending it to reviewers, or even select a known harsh reviewer for a paper to see that it is rejected (defined as subjectivity [25]). Alternately, an editor-in-chief might specifically discriminate against authors as they deem appropriate, perhaps, because of host institution or because of research that does not support

his or her own philosophy (considered bias [25]). As an example, the present authors have recently published their own Evidence-Based Resistance Training Recommendations (6). Our own partiality is, alas, already evident in that we failed to even consider journals that have repeatedly presented opinions opposing our conclusions. Thus, in the belief that we would ultimately be rejected by editors and/or reviewers based on our findings (disguised as perhaps 'writing style' or the like), we selected another journal for submission and publication.

Even more recently the present authors, with another topical review contacted an esteemed journal, believing our article to fall within their scope. The editor refused to even accept submission. As a result, in discussion with the other authors to approach an alternate journal based on impact factor, our responses were that we would rather submit to a journal that we believe to be more authentic in nature regardless of impact factor, based on the urgency of publication of such a review article (e.g., that a lengthy peer review process might serve to out-date the article). Of course, if we were to follow that process, we would become violators of our own criticisms. We would be showing favoritism towards a journal which we perceive more scientific in its processes. Ultimately, we submitted to a journal that would accept uninvited reviews and (while receiving valuable feedback) the article was subsequently rejected. We have since had more success in response to our manuscript including its submission to another journal. However this should not detract from the point made by our initial encounters; that dissenting views may be stifled before even a review process occurs. Martin (12) discusses these comments as the search for an "open-minded scientist."

The paradox of this process is that which might best be described as territorialism. Somewhat understandingly, authors who have made a living from providing evidence and supporting opinions, might be as resilient to new articles as those who oppose them, arguing that there is little place for new research or that it does not add to the current research base. Authors might be resistive to see their field of work diluted by the influx of new research or researchers. Of course if we are fearful of potentially opposing authorities and find little to no sanctuary in like-minded researchers, then, our opportunity for publication might be considerably diminished.

THE FILE DRAWER PROBLEM

Publication bias has been deemed to exist for some decades. Thornton and Lee (23) expose the awareness of publication bias as dating back to an editorial in the *Journal of Abnormal Social Psychology* in 1956 with the comment that "negative studies were less likely to be published in his journal." In 1979, Rosenthal (17) openly discussed what has become referred to simply as the "file drawer problem." Meaning, "that the journals are filled with 5% of the studies that show Type I errors, while the file drawers back at the lab are filled with 95% of the studies that show non-significant (e.g., P>.05) results". This in itself represents a potential bias in scientific publication; that a collective of studies provide no evidence of a relationship or of cause and effect and therefore are not submitted for publication. Of course, worse yet is the potential that such articles are submitted but later rejected from publication because they reported findings that failed to reject a null hypothesis; perceived as less important than other articles (19,22).

IMPACT FACTOR

Within the publication process we should also consider impact factor (IF); often mistakenly considered to represent the 'quality' or at the least comparative reputation, of a journal. Many scientists believe that these are the opinions held by financing agencies supporting scientific research and by educational and research institutions when selecting their personnel. In fact, IF simply denotes

the average number of citations per citable article (editorials and letters to editor are not usually considered) within that journal (7). Amin and Mabe (1) warn of the use of IF to rank journals, as well as advising against its use as a measure of quality. In consideration impact factor potentially holds significantly less importance than first considered. The quality of article published in journals with lower impact factor likely hold no less value; the peer review procedures appear no less strict and the scientific processes employed by the articles contained are of equal caliber. And yet, by our experience many authors strive to be published within journals with higher IF. Does the journal in which an article is published somehow make the conclusions more 'true' or the underpinning science more accurate, or reliable? Based on the questioning title of this article perhaps *authority* might be a reference to not only author but publishing location.

JOURNAL ARTICLE ACCESS

As an additional complication to the consideration of IF, we now have the contemporary inclusion of open access[2] (OA) journals. McVeigh (13) suggested that the largest number of OA journals exists in the Medicine and Life Sciences, and indeed the growth of OA journals and the comparative IF between OA and non-OA journals is discussed by Harnard and Brody (8), comparing well for OA journals (albeit not in the specific discipline of exercise physiology). Of course, OA journals have a possible advantage in the potential to be read by a larger audience, but questionably – is it the right audience? In fact, the present authors have recently received suggestions from an experienced colleague to avoid publication in OA journals *because* of the availability (specifically to the non-scientific community); for fear that articles might incur or induce negative feedback or the misinterpretation and thus, be discussed unfavorably in the non-scientific media (e.g., websites, blogs, etc.). Ultimately, the advice received and the decision to make available by submission to OA journals is an elective for the authors, but without question differing perceptions exist which might bias future publication, attainment of funding, and even position or stature within an institution.

We also might consider that to presume no value can be gained from the non-scientific community is utter arrogance. The likes of science journalists such as Gary Taubes have provided mainstream media (in the form of both articles in Nature and the New York Times as well as popular science books) with vast underpinning scientific support, questioning prevailing paradigms and deep-rooted thinking on subjects such as nutrition as well as others by the so-called *experts*. Shanteau (20) critically discusses the concept of 'experts,' highlighting the use of *"automated thinking"* (21) by experts, and *"controlled processes, which are linear and sequential, more like deductive reasoning"* for novices (11). Indeed Thomas Kuhn (10) in his famous work, *"The Structure of Scientific Revolutions"* considered that normal science, what Shanteau (20) describes as involving automated thinking, utilized this process in continuing under existing paradigms with little to no questioning of the paradigm currently being labored under. For example, a person with a foundation of knowledge might seek only to expand upon their existing knowledge, rather than ever question the founding theories or methods they utilize. Of course the study by Prasad et al. (14) suggests that this might not be the case, whereas the continued thinking by the ACSM might be seen to suggest otherwise, at least, within our discipline. Kuhn (10) went on to suggest that those who challenge and succeed in shifting scientific paradigms are *"either very young or very new to the field whose paradigm they change."* We believe this is also applicable to those non-experts or 'armchair scientists' from the non-scientific community. Boone (2) comments that to challenge existing paradigms is difficult particularly where

[2] Open Access Journals are considered those that do not charge the reader or his/her institution for the right to access, download, copy, print or distribute.

there may be repercussions (e.g., rejection from publication). Boone (2) clarifies this point in the article title, *"leading change requires guts."*

SUMMARY

Amidst this smog that clouds the scientific process is the hope that as the scientists of tomorrow we can learn from the potential limitations discussed. We do not propose a conspiracy theorists approach toward scientific publication, but rather encourage an understanding and critically evaluative attitude. We can hope that by exposing and discussing these potential biases that we encourage reviewers, editors and scientists alike to retain nothing more than an attachment to scientific process and a search for the truth, whatever that might be, rather than previous research or a set of beliefs. Respectfully, we believe the unbiased processes considered are invaluable in scientific publication, but that both the perceptions and the evidence presented from other authors herein, supports that these limitations exist and need consideration.

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